Bromine.

When the experiment with the pressure gauge is made with bromine instead of iodine, it is found that there is a considerable increase of pressure produced by the passage of the spark, but that this disappears almost as soon as the sparking, and on determining the vapour-density of the sparked and unsparked bromine it is found that they are identical. It seems most probable that the difference between bromine and iodine is not that the bromine is not dissociated by the spark, but that the atoms combine very much more quickly than the iodine atoms. The vapour-density determinations showed that bromine vapour is dissociated if it is heated for a long time at a low pressure, even though the temperature is not very high.

The results of these determinations are given in the following table:—

Pressure.	Temperature.	Density.	Remarks.
473	111	80	
466	106	81	
430	101	80	
602	116	79	
543	89	81·7	In bath for 24 hours.
315·5	105	73	
235	109	77	*Sparked.*
230	100	66·5	In bath for 4 hours.
165	90	77	Only a short time in bath.
390	111	70	In bath for 7 hours.

These experiments show that it takes a long time for bromine to reach a state of equilibrium, and that for the experiments on the vapour-density, the gas should be maintained at a constant temperature for some time before the experiments are made.

Experiments on chlorine and nitrogen tetroxide are also described in the paper.

II. "On the Supposed 'New Force' of M. J. Thore."* By William Crookes, F.R.S., Pres.C.S. Received May 5, 1887.

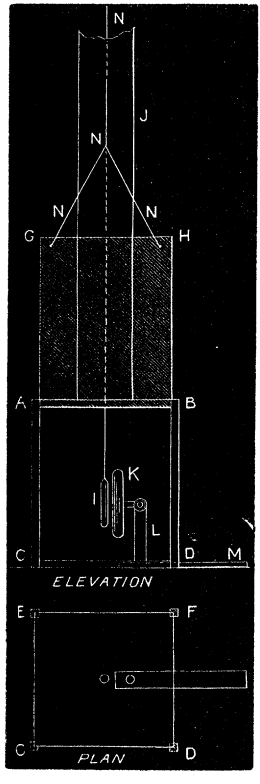
(Abstract.)

The author commences by quoting the description of some apparatus and experiments which have led M. Thore to suspect the existence of a new force inherent in the human organism. M. Thore suspends a

* 'Une Nouvelle Force?' Par J. Thore. Dax, 1887.

small cylinder of ivory by a fibre of cocoon silk, forming a small pendulum, which hangs freely over the centre of a table. The cylinder having become motionless, M. Thore brings a second cylinder, called the "pillar," about a millimetre from the first cylinder, when the latter begins to rotate clockwise if the pillar is on the left, and counter-clockwise if the pillar is on the right of the cylinder. The observer is supposed to face the cylinder and pillar. M. Thore says that the rotation is independent of the nature of the cylinders, of their mass, or the dimensions of the pillar; light, heat, electricity, magnetism, gravity, and air currents, he says, are also inadequate to explain the phenomena.

The author has repeated M. Thore's experiments in apparatus shown in the accompanying figure. It consists of a glass case, A, B, C, D, E, F, $6\frac{1}{2}$ inches square and 7 inches high, with a rising glass window, A, B, G, H, in front and similar windows at the sides. The top is of card, in the centre of which is a small hole. The cylinder, I, is suspended in the middle of the case by a very fine cocoon silk fibre, 5 feet long,



surrounded by a card tube J, attached to the top of the glass box. K is a second cylinder attached to a support, L, M, by a ball and socket joint for convenience of adjustment. The support, M, projects outside the case to admit of the *pillar* being brought close to the *cylinder* and transposed from one side to the other, &c. N is a cord attached to the front glass window, weighted at the end and passing over a pulley for convenience of raising and lowering the glass.

Ivory, ebonite, glass, and metal have been used for the cylinders and for the pillars. The pillars have also been made square, round, and wedge-shaped in section, and the surfaces have been bright and lamp-blacked. The mode of experimentation is the following:—The cylinder being at rest, the observer sits down in front of the apparatus with his face 8 inches from the cylinder and pillar, taking precautions to keep the breath as much as possible away from cylinder and pillar. The pillar is always placed on the right of the cylinder. On raising the front glass the cylinder commences to rotate in the opposite direction to the hands of a clock, the side nearest the observer moving to the right.*

In other experiments a flask of boiling water, a candle, and a hot platinum wire have been used as the source of radiation. The results of more than fifty experiments are given in tables, showing the material of the pillar, the maximum speed of one revolution, the number of revolutions, and the exciting agent. Experiments tried with an ascending current of air of different degrees of intensity in front of the apparatus prove that air currents are inoperative in producing the action.

The results leave little doubt that the action is one of radiation from the face or other warm body in front of the apparatus, and that there is nothing special in the human organism beyond the heat it radiates to produce rotation of the cylinder.

Radiant heat (and in less degree light) falling on the lampblacked surfaces is absorbed, and increases the surface temperature. There are two ways in which this increase of temperature may act:—

1. It may produce a current of warm air, rising in front of the surfaces of the moving body; to replace this, cold air will come in from all sides, and striking against the delicately suspended cylinder cause it to rotate. If, however, the source of heat is of considerable surface, such as the face or a Winchester quart bottle full of warm water, it is difficult to imagine that there will be much tendency to rotate in one direction rather than in the other.

2. An increased surface temperature of the cylinder and pillar may produce an increase of molecular pressure between the two bodies,

* This is called the *negative* direction, and when the rotation is clockwise it is called *positive*.

and thus give rise to motion, after the manner of the radiometer. In this, as in the former case, the movement should be in the opposite direction to what it is in reality, as it would be produced by mutual repulsion acting between the sides nearest the source of heat.

It seemed likely that information, decisive as regards one or other of these two theories, might be gained by suspending the cylinder in a glass tube attached to a Sprengel pump, and taking observations at different degrees of exhaustion.

In experiments tried by the author in 1875* the noteworthy fact was ascertained, that two bodies of different temperature attracted each other at normal atmospheric pressure; the attraction rose as the pressure diminished, until, at a tension of 1·15 mm., it was nearly four times what it was in dense air. Above this exhaustion the attraction suddenly dropped and changed to repulsion, which at the best vacuum obtained was nearly thirteen times stronger than the attraction in air.

Two forms of apparatus are described by the author, wherewith experiments were tried during exhaustion, and an exact parallelism was established between the attraction or repulsion of the cylinder by a hot platinum spiral, and the positive or negative rotations of the cylinder under the influence of a warm body brought near.

The two phenomena run absolutely in parallel lines; when there is attraction negative rotation is also produced; when the exhaustion is such that the attraction is *nil*, the rotation is *nil* also; when the attraction changes to repulsion the rotation changes from negative to positive; and when the vacuum is very good, so that the repulsion between the two heated bodies is at its maximum, then also the positive rotation is the strongest. It is impossible to resist the conclusion that the two sets of phenomena are due to the same cause, and that as air currents did not produce the attractions observed in the 1875 experiments, so likewise are they equally inoperative in giving rise to the present rotations of the suspended cylinder.

If the rotation is produced by a reaction between the suspended and fixed body, it follows that were both free to move each would rotate, but in opposite directions. To test this, another apparatus was made, having two delicately suspended cylinders, 1 mm. apart, in a glass tube capable of being exhausted. In a table the results of twenty-two experiments are described, observations having been taken at intervals during exhaustion. Down to 14 mm. pressure the two cylinders rotate negatively (*i.e.*, the right hand cylinder rotates clockwise, and the left hand cylinder counter-clockwise). Between 14 and 3 mm. there is no rotation, and below 3 mm. the rotation is positive, the movement at an exhaustion of 0·0495 mm. being five times as strong as it was originally.

* 'Phil. Trans.,' 1875, Part II (pp. 528–532).

The motive force producing these rotations is, at high exhaustions, the molecular impacts between adjacent surfaces of the suspended cylinders excited by the radiation falling on them from the hot water, hot spiral, or a candle (which is equally effective). But what produces the negative rotation at ordinary atmospheric pressure? *Air currents* are the obvious explanation, but there are grave reasons for believing this explanation inadequate. In the first place actual air currents when tried do not produce the desired result. Secondly, it is most logical to assume that as the present set of experiments are strictly parallel with those tried in 1875, and as in each case the results at high exhaustions are due to molecular bombardment, so also must the similar results at low exhaustions be due to the same cause.

Finally, twenty-one experiments in the form of a table are described, in which an apparatus was employed, specially designed to eliminate the interfering action of air currents, and submit the molecular bombardment theory to crucial experiments. The results are considered by the author as conclusive in favour of this explanation.

Addendum, May 24, 1887.

I sent M. Thore a detailed account of my experiments, asking him to favour me with any comments or remarks he might wish to make. I have just received a long communication, partly printed and part in MS., in which he describes many fresh experiments, and adduces arguments to show that my dynamical explanation is not sufficient to account for more than a few of the facts he describes, and saying that he "persists in still believing that this force emanates from the observer, or else that the observer is the indispensable intermediary for its manifestation."

The experiments are numerous and are devised with great ingenuity. It is impossible in the space of a brief abstract to do more than refer to a few of the principal facts here brought forward. M. Thore commences by objecting to my having experimented in an enclosed space, saying that he always operates in free air. He thinks that enclosure may almost or quite suppress his force. To this I can reply that I have myself verified nearly all M. Thore's facts of rotation (including those just now communicated), when working in the free air of a large room, and it was only when I found the delicacy of the observations was impeded by draughts and currents that I put screens round the apparatus. I have not found glass screens interfere materially with any of the rotations. M. Thore now says that it is necessary to hold the pillar or the exciting body in contact with the hand during the whole duration of the experiment. I

was not aware that importance was attached to this point, but I have since repeated many of my former observations, holding the pillar in the hand. The results are certainly stronger, but the extra heat imparted to the apparatus is in my opinion sufficient to account for this. M. Thore brings forward many new and ingeniously devised experiments to prove that heat cannot be considered the cause of the movement. He exposes the instrument to the full sun and then brings it into a cool dark room; he suspends it over boiling water; he places a large block of ice between the cylinder and the observer; he similarly interposes metallic vessels full of boiling water between the cylinder and observer (the observer not moving from his place in front), and he tries the experiment in a hot chamber alternately moist and dry, without finding the regularity of the movements interfered with. I have tried most of these, and obtained results corroborating M. Thore's, but I have also tried the experiment of quietly bringing near to the stationary cylinder a bottle of hot water and observing the movement from a safe distance through a telescope, and I find that the hot bottle is able to effect rotation as well as the observer.

Among the curious observations mentioned by M. Thore is this:—Placing the pillar in front of the cylinder (between it and the observer), if the pillar is held with the right hand the movement is clockwise, and if the left hand is used the rotation is counter-clockwise. The right hand is stronger in its effects than the left hand in the proportion of 2 to 1.

M. Thore has given in addition a large number of curious and interesting observations, using two, three, and more movable cylinders and recording their movements under a great variety of circumstances. I admit I do not see at once how all these are to be explained on the molecular bombardment theory. But this theory has not yet explained all the anomalous results I have recorded in my papers on "Repulsion resulting from Radiation," although I believe it capable of doing so; and I therefore think that it is not necessary to call upon a new force to explain any of M. Thore's results which radiation does not yet seem able to account for.

The Society adjourned over the Whitsuntide Recess to Thursday, June 9th.

Presents, May 26, 1887.

Transactions.

Buckhurst Hill:—Essex Field Club. The Essex Naturalist. No. 4. 8vo. *Buckhurst Hill* 1887. The Club.

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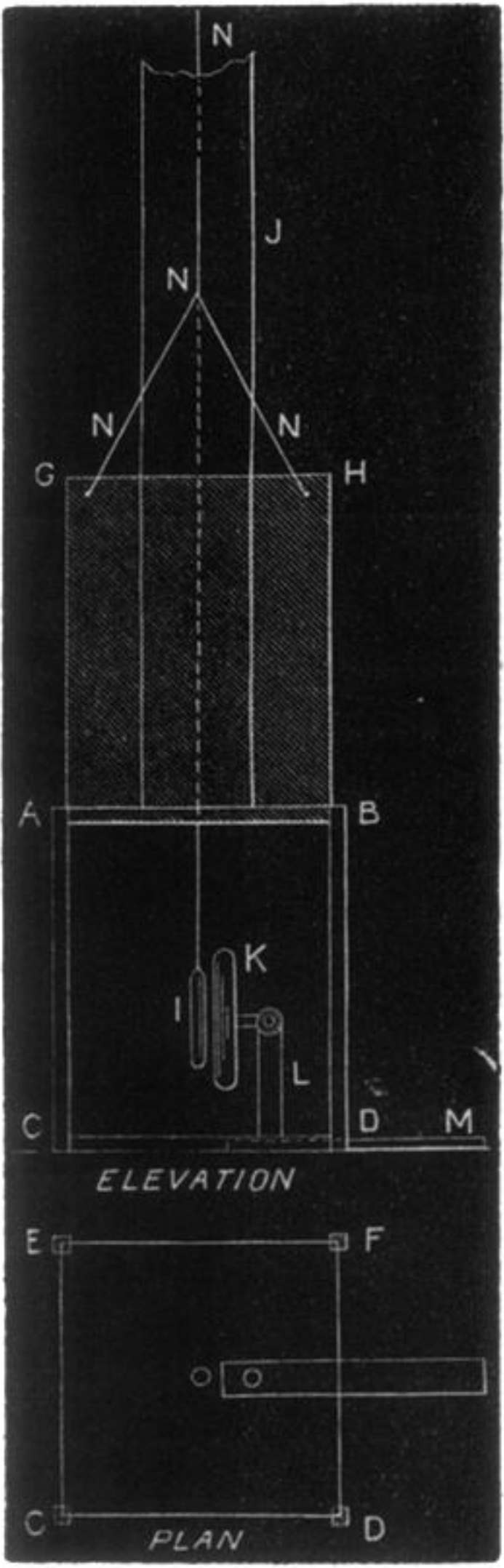
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